Cabell (J. L.)

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STATE MEDICINE AND PUBLIC HYGIENE

BEFORE THE

AMERICAN MEDICAL ASSOCIATION, JUNE 6, 1878.

BY

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UNIVERSITY OF VIRGINIA.

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INASMUCH as a special ordinance of the Association makes it the duty of your Permanent Secretary to report annually the names of States where boards of health exist, and also of those which decline to establish them; said report to form a part of the annual proceedings of the Association, I am thereby relieved of this portion of my duty in relation to the advances in State Medicine during the past year. I cannot, however, forego the opportunity of saying that the establishment of a general board of health is the indispensable initial measure of any systematic effort to inaugurate State Medicine in any of the States of the Federal Union. What has been accomplished in this direction during the past year will be reported by the Secretary in conformity with the requirements of the ordinance to which I have alluded. What may be effected towards the protection of the health of communities by judicious sanitary legislation conceived and subsequently carried into execution by the sanitary advisers of the State is strikingly exhibited in the result of an obstinate contest of the butchers of Boston with the State Board of Health, a contest which continued for five or six years, but which finally terminated in a complete triumph for the board by the abatement of a nuisance of great magnitude and of an extremely dangerous character, affecting the populous town of Brighton, which thereafter became a salubrious and agreeable place of residence. Dr. Bowditch, President of the Board, has stated that it required a "thirty years' war" before the legislature of Massachusetts was conquered, but in view of what has been effected by his board, assuredly the fruits of this victory have amply compensated for all the labor expended in the protracted contest, and the lesson is one by which others may profit.

The first step taken by the newly created board was to address a circular letter to the civic authorities of every city and town, to every member of the legislature, and to every physician and clergyman in the commonwealth, inviting their sympathy and co-operation, and especially soliciting the nomination of "a medical correspondent in every town to whom they could apply for local information," some physician possessing the confidence of the local community who would be willing for the public good to report to the board facts relating to disease occurring within the jurisdiction of his town. I cite this fact, which as relating to voluntary services of private individuals may at first view seem to have but little bearing upon the question of State Medicine, not only because such services, solicited by the official sanitary advisers of the State, were designed to be in aid of the newly inaugurated system of State Medicine, but also and chiefly because it serves to exemplify one of the ways in which the ground may be prepared for State action.

It was, doubtless, foreseen by these earnest pioneers in the work of inaugurating State Medicine in America, that legislation was little likely to go ahead of popular opinion and consent, or to be of much practical utility if it were thus pressed forward, and they, therefore, recognized the importance of directing the attention of the citizens to well-authenticated and significant facts collected in their respective communities by physicians in whom they could confide. Then, it was hoped, the demand for sanitary legislation would come up from the people themselves.

Now, as to the value of sanitation in general in increasing the salubrity of localities and diminishing the death-rate, some very remarkable illustrations recently stated in a public address by that eminent lay sanitarian, Mr. Edwin Chadwick, may be cited here as an exponent of the latest "advances and discoveries" in the practical applications of sanitary science. This address "On the Present Position of Sanitary Science, for its Future Practical Advancement," was delivered by Mr. Chadwick as President of the Health Section of the British Social Science Association, at Aberdeen, September 13, 1877, and contains many striking and suggestive facts which, so far as I know, have not been published elsewhere. He has successfully shown that a Norma of Sanitation for the infantile stages of life may be established with factors that go to the prevention of an annual excess of upwards of 50,000 deaths in the school stages of life, in England

and Wales. The details of this statement are so interesting and important as to justify the recital somewhat *in extenso*.

"In 1838 there fell to the direct administration of our Poor Law Board, of which I was secretary, two large institutions for the care of destitute orphan children, which suffered severely from passing epidemics of typhus fever. In one, at Norwood, containing 700 children, there was a severe outburst of typhus fever, by which more than one-third were attacked and more than thirty (30) were killed. The general declaration of medical men at the time was, that the mortality was occasioned by deficient food. But the food was better and more abundant than the food of the independent wage classes. I got the case examined by the late Dr. Neill Arnott, who was a specialist in one element of sanitation, viz., ventilation. He pronounced the main evil to be not deficiency of food, but deficiency of pure air, and the remedy was the constant removal of putrefying matter by good drainage, and of foul air by ventilation. This was adopted, with the result of a reduction of the death-rate by about one-third. Next followed the production of clean skins, by regular head to foot ablutions with tepid water; and this was followed by the reduction of about another third of the ordinary death-rates. Other improvements in detail have followed chiefly in physical training. As sanitary improvement has advanced in these institutions there have been fewer inmates of the sick wards, to the extent that not one-fourth of the beds provided for sickness are now actually occupied. The particular institution first attacked, and now eight other large orphan institutions, district asylums, are in fact children's hospitals, where numbers are received only to die. All the inmates as a class are of the most wretched type of children, all weakly and susceptible to disease; but of those who come without marked disease, there is now an almost absolute immunity from the children's epidemics. Cases of typhus, at one time scarcely ever absent, have not been seen there for several years. *The mean death-rates in these institutions have been steadily reduced to about* 3 *in* 1000, *that is to say, nearly one-fouth of the general death-rate of children of the school ages, including the children of the well-to-do classes of the population.* It was recently stated, as evidencing the success of the "boarding-out" system, that the deaths had not exceeded 2 per cent.; and this may be taken as an average children's death-rate for the cottage, that is to say, 20 per 1000, as against 3 per 1000 in the district orphan institution, with little variation in the separate institutions. Medical officers in charge of them, but who are in private practice, have repeatedly expressed to me their astonishment at these results of sanitation as surprising and wonderful to them. I was lately present at a prize given to agricultural laborers by Lord Shaftesbury, where one prizeman was a shepherd, who, out of 100 lambs he had reared, had only lost one. Such emulation may well be directed to the preservation of human life in infancy. On a visit to one of these orphan institutions, I told the governess that the Queen of the Belgians had offered a gold medal to whichever manager of such institutions should rear the greatest number of infants, and I asked the governess whether she would compete in conservancy of life against that shepherd with his lambs. She proudly declared that she would do more than that, and with the infants of the ages under her care she really has done more."

Mr. Chadwick adds the very significant and suggestive fact that

the schools in which such remarkable results have been obtained, are "on the half-time principle, introduced with the Factory Acts as preventive of the physical injury done by over-sedentary work in the long time schools, and that by a better adjustment of the book teaching to the children's mental powers of receptivity, and by the freshening up of the faculties by brisk gymnastic exercises and the military drill, these children, though naturally of an inferior mental type, are got well through the three R's in less than four years as against seven in the common elementary schools, and at half their cost for teaching power; and with the economical outcome, that the efficiency of three is imparted to two for industrial occupations."

He then proceeds to show that an equally favorable norma of sanitation may be attained for the adult stages of life of the ages of the prison populations, men being taken from the ranks, where the death-rates were 17 per 1000, and put into prisons where the death-rates were only 3 in 1000, and that these results were obtained by very rudimentary means and are evidently susceptible of further advances.

Adverting to the decimation by reason of insanitary administration of the first army sent by England to the Crimea, and the saving of the second army as a result of the sanitary measures instituted by a special Royal Commission sent out for the purpose, that army returning in a better condition of health than the army at home, Mr. Chadwick comments upon the failure of the government to recognize by suitable rewards the services of the able men who had accomplished such a saving of valuable lives. "It marks," he says, "the antipathy, or at least the apathy, with which sanitation has to contend—of which other large examples may be adduced—that whilst every official through whose misfeasance the first army was lost was decorated, not one of those through whose sanitary science the second army was declared to have been saved received then or since the slightest notice for the achievement."

After citing statistical records to show the extremely satisfactory results of sanitation in the British army and navy, the Indian army, for example, of which the death-rate, according to the report of a Royal Commission of Inquiry as late as 1859, had since the first occupation of the country oscillated around 69 in 1000, having now a death-rate of only 17.48 per 1000, Mr. Chadwick argues that it is perfectly practicable to obtain favor-

able normas of sanitation by local or private effort for the protection of civic populations. Thus in the blocks of model dwellings in London erected by voluntary associations the death-rates range separately from 14 to 17 per 1000, which is an important advance on the general death-rate of 23 in 1000. But the real gain is far greater than is indicated by this comparison with the average death-rate of a large city. The comparison should rather be made with the death-rate of the ordinary dwellings occupied by the wage classes, which is very far above the general average.

Similar testimony is borne by Dr. Russell, medical officer of health of the city of Glasgow, who states that "in the large model lodging-houses of the corporation a case of fever is the greatest rarity and never spreads. In the common lodging-houses belonging to private individuals we have fever oftener, but there again it never spreads; that is to say, as it does in the defective houses of the wage classes." Adverting to Mr. Chadwick's norma of sanitation in prisons, Dr. Russell is convinced that if he had perfect command over all the Irish in Glasgow, as the governor of a prison, he could reduce the death-rate fully one-half in five years, or in less time than that.

On a full survey of all the facts obtained from recent returns, Mr. Chadwick feels warranted in affirming, that by vigorous and thorough sanitation the general death-rate of London might be reduced from 22 to at most 17 per 1000, and that of the provincial cities in the manufacturing districts in yet greater proportion.[1]

Equally significant examples of the beneficial results of sanitation in general might be cited to an indefinite extent, but these may suffice. Special sanitary reforms require a previous knowledge of the special nuisances to be abated, and this knowledge is obtained or verified by mortality and sickness statistics. One of the early measures to be inaugurated by a State Board of Health relates to a proper and effective system for the collection of statistics of disease and death throughout the entire State, a measure which is obviously impracticable otherwise than under the sanction and authority of the State. Registration of deaths

[1] Mr. Chadwick concluded the Address referred to in the text, by a valuable summary of the chief results obtained of the progress of the power of sanitation, which is reprinted in *The Sanitarian*, of New York, of June, 1878.

and of the causes of death, inestimable as such registration may be, will not, however, answer all the requirements of Preventive Medicine. By means of mortuary statistics we may make the disasters of the past available as monitors to warn and instruct us as to the means of averting their recurrence in the future, but they are gathered too slowly to avail for contemporaneous protection. It is only by the registration of current diseases that we can hope to secure the application of prompt and efficient measures in the interest of special sanitation. Moreover, it is not exclusively, nor even, perhaps, mainly the mortal diseases which entail affliction and pecuniary loss upon the individual citizens and constitute a heavy burden on the State, for many diseases which seldom or never prove fatal do yet rob life both of its joy and its usefulness, and effectually destroy its working and productive energies. It is, therefore, of the first importance to have an accurate record of existing disease as early as possible after its appearance in any locality in which it is desired to establish the most favorable norma of sanitation.

A general and complete registration of diseases has not yet been found practicable in Great Britain, or in any of the States of our Union. A praiseworthy attempt to inaugurate a plan of reporting the prevalent diseases of the State was made by the State Board of Health of Massachusetts in 1875, in conformity with the suggestions of Dr. F. W. Draper. It was hoped that by the voluntary co-operation of a large number of regularly educated, intelligent medical men, whose field of practice was so distributed as to enable them in the aggregate to give a comprehensive and accurate weekly conspectus of the diseases prevalent in the entire State, to obtain a sufficient number of returns each week to admit of useful generalizations. In the 8th Annual Report it is stated that a little more than one-half the number who agreed to co-operate sent in their reports each week from about ninety out of the three hundred and forty one cities and towns of the State. Unfortunately, too, for comparison, the physicians not reporting were not the same from week to week.

A similar experiment is now in progress in Michigan under the auspices of the State Board of Health, with what degree of success we are not yet informed.

Dr. B. W. Richardson referring, in an Address delivered before the Sanitary Institute of Great Britain, July 5th, 1877, to an effort he had made many years ago to carry out a plan of regis-

tering the diseases of the Kingdom, states that in a short time fifty medical observers were sending in returns of epidemic diseases, to which his plan at that time was limited, from as many stations, which could easily have been increased to any extent.

It was during this period that the first account of the invasion of England by diphtheria was contained in the returns from the district of Canterbury. "This disease first appeared in the little village of Ash, and was called the Ash fever. The first facts of a new disease were thus recorded on the spot which," says Dr. Richardson "is something even as a matter of history. How such a fact, reported at once to a central government authority, might be dealt with; how promptly a central authority so advised might act in arresting a fatal epidemic at its origin, and what national service is rendered thereby," it is not difficult to conceive. He then urges the importance of pressing this scheme for the registration of disease on the government, and thinks that the Sanitary Institute may greatly assist the work by lending its mind to the best means of collecting the facts on which the weekly reports of disease will have to be based.

It appears from the concurrent testimony of all who have inquired into the subject that the registrations of deaths in the several States of this Union, and consequently of the whole country, is extremely defective. Even in Massachusetts under a system closely similar to that which in England has reached so high a degree of perfection, the State Board of Health has recently reported very glaring defects, and Dr. Henry B. Baker, the able and earnest Secretary of the State Board of Health of Michigan, has declared that "no method has yet been found, or at least acted upon, whereby the actual death-rate can be positively ascertained for the United States, or, so far as he knows, of any single State."

Now, here is a crying evil for which legislation is urgently needed. If it be "not too much to say that modern sanitary science owes its existence to the registration of deaths and their causes, and the localization thereby of insanitary conditions," the importance of complete and accurate records can scarcely be exaggerated, for death returns are, in default of yet more general sickness returns, the basis of all sanitary statistics. It can be so easily shown by the irresistible logic of statistics that no amount of taxation likely to be called for for sanitary improvements can be half as burdensome to a community as the losses entailed by

preventable sickness, and that consequently the outlay required for sanitation is the most remunerative investment which a community of tax-payers could possibly make.[1]

Let us, therefore, not relax our efforts until in all the States of the Union, we shall have obtained results equal to those realized in Great Britain by the Public Health Act of 1875, which Dr. Richardson characterizes as "splendidly comprehensive."[2] Notwithstanding her pre-eminence in matters of State Medicine the leading sanitarians of that great Empire are never weary in their efforts to popularize the knowledge of sanitary principles and practice, an example which, I rejoice to know, is closely followed by those zealous pioneers in the cause of sanitary improvement who started the scheme of an American Public Health Association and have done so much to impart interest to its annual meetings.

In view of the actual and prospective success of such systematic sanitation in England, some writers in that country seem to apprehend an overplus of population and to entertain doubts whether there was not more harm than good in such success in sanitary work. "Medical science," says Mr. Grey, "is mitigating

[1] "The total annual loss and cost occasioned by illness from zymotic diseases in this country (England), if it fell upon the wage class alone, would amount annually to more than £2,500,000. Then there was the loss from imperfect recoveries, such as eye-diseases following measles, blindness following zymotic diseases. If such evils were to fall for the first time upon a community paying heed to sanitary regulations, scarcely any effort would be thought too great for their removal. It was only the deadening influence of habit, which enables so many among ourselves to regard them with comparative indifference. It would be too much to say that all zymotic diseases could be removed by proper legislation and adequate care; but past experience was distinctly in favor of the proposition."—R. Brudenell Carter, "On the Present Possibilities of Sanitary Legislation," *Sanitary Record* of October 12, 1867.

[2] In giving a rapid summary of the beneficent provision of the Act, Dr. Richardson specifies "the constitution of sanitary authorities throughout the Kingdom; the power it invested in those authorities to appoint learned medical officers of health, the provisions it made for securing to each locality better sewerage, freedom from nuisances, improved water supply, regulation of cellar-dwellings, governance over offensive trades and removal of unsound foods; the provisions for prevention of spread of infection, and for the erection of hospitals and mortuaries; and the provisions for the regulation of streets and highways, lighting of streets, establishment of pleasure-grounds and regulation of slaughter-houses; these as well as the general provisions for the carrying out of the Act, were, in his opinion, most commendable as practical plans by the working of which the nation may be tempered into a sanitary mould of thought and character."—*Nature*, July 5, 1877, p. 185.

suffering and achieving some success in its warfare against disease; but at the same time it enables the diseased to live. It reduces the aggregate mortality by sanitary improvements and precautions; but those whom it saves from dying prematurely, it preserves to propagate useless and imperfect lives."

Dr. Ransome in quoting this remark gives an overwhelming answer from Mr. Grey's own point of view. "It appears," he says, "to be forgotten that most of the evils that we deplore in our town populations, the disease and feebleness from which they suffer are the direct outcome of the unhealthy circumstances in which they live. If the conditions of existence in towns continue as they now are, if the fever nests, the breeding places of scrofula and consumption, the temptations to intemperance and vice, are left unchecked to do their malignant work, then indeed we may certainly look for deterioration of race, but sanitarians will not be responsible for the result."[1]

In the last volume of the Transactions of this Association, an excellent paper on the "Relation of Heredity to Race Degeneration and Improvement," by Dr. Black, of Ohio, contains the records of a number of typical instances of recovery or reversion from inherited tendency to disease, when the conditions favorable to such reversion to the normal state have been faithfully and skilfully carried out.

Then, as to the Malthusian fallacy, a sufficient refutation had been given by Dr. Wm. Farr, who, from his masterly deductions

[1] Never was man more belied than Malthus is by his new-found friends. He never, like them, proposed to rest satisfied with a high rate of mortality, and to look calmly on while this modern massacre of innocents was taking place, for he says "a decrease of mortality at all ages is what we ought chiefly to aim at." And again "a young person saved from death is more likely to contribute to the creation of resources than another birth. It is a great loss of labor and food to begin over again."

But all the preventive checks named by him, except the prudential check, must inevitably lead to a great deterioration of race. The checks that he never really sanctioned, but with which our friends are apparently afraid to interfere, pestilence, unwholesome trades, misery, and vice, all these necessarily lower the standard of life and endanger the future well-being of the nation. The prudential check to population would, of course, be a splendid invention, if it could be prudently regulated, or if individual judgment could be trusted. It is still a question how far restrictions upon marriage can be imposed by the State without harm; but if the matter be left to individuals, as Galton has shown, only the improvident will breed and the race will infallibly become of a lower type."—Dr. A. Ransome, "On the Present Position of State Medicine in Great Britain," *Public Health*, August 24, 1877.

drawn from the vital statistics of England, concluded that "the great source of the misery of mankind is not their numbers, but their imperfections, and the want of control over the conditions in which they live." In like manner Dr. Nathan Allen, of Massachusetts, in a series of admirable papers published during the last few years, has conclusively shown that the decrease of the native element of the population of New England cannot be due to the causes usually assigned for such an effect, since these causes have scarcely occurred, and if they had occurred, would have operated on the foreign as well as the native elements of the population. He gives good reasons for believing that the effect is to be explained by a gradual change in the physical organization in both sexes, "in the case of the males in giving up manual labor, physical exercise, and out-door work generally, for lighter employment in the shop and business that taxes the brain far more than other parts of the body. In the case of females giving up housework and employment which make a severe demand upon the muscles, in giving more attention to mental pursuits, to the fashions of the day, to the artificial habits of society and what may be called the luxuries of modern civilization. These and other causes, commencing in early life, have served to impair not only the strength or stamina of the constitution, but to change the *harmony or balance of the organization*: that is, instead of having a good development of the muscles and lymphatic temperament, we have a great predominance of nerve tissue, so that the constant demands of the brain are far greater than the physical system as a whole can bear."

PUBLIC HYGIENE.

The evidences of an advance in Public Hygiene with reference to the maintenance of the purity of the air within and around dwellings consist not so much in the discovery of new facts or principles, as in more careful, exact, and honest methods of sanitary engineering, in conformity with well-known laws of sanitary science. It can scarcely need to be said that one of the most fruitful sources of impurity of the air around dwellings and consequently of the air within dwellings, since the latter is constantly being replaced by the former, is the damp condition of the ground, which however well drained of subsoil water at first is liable to subsequent contamination with liquid filth whether arising from

slops thrown upon the surface of yards and gardens, or from the dejections of animals. A soil well aërated by proper underdrainage would, in a measure at least, oxidize the organic matters and mitigate the generation of malaria. It would not, however, be safe to rely upon this to disinfect the excrement-sodden soil in the streets of cities, and it becomes an important problem of sanitary engineering how to protect the atmosphere of cities from this source of contamination. This end will probably be secured by the best asphalt pavements, such as have been largely used in some of the cities of Europe, and have been successfully introduced into some parts of Brooklyn, and on the principal avenues of the national metropolis. In addition to the other valuable qualities of firmness, noiselessness, durability, with facility of removal when necessary for laying water or gas pipes, and equal facility in repairing the breaches thus made, their non-combustibility, and finally their comparative cheapness, they are also clean and impervious. No water or filth can penetrate them, and they are easily cleaned at less expense than any other pavement. A necessary condition, however, of the beneficial action of impervious pavements, is the absolute freedom of the subsoil from the sewage contamination which might arise from defective sewers or from percolation of the contents of cesspools. The late Dr. Parkes, in his report on hygiene for 1873, suggested that a peculiarity of house construction in Munich, which may have an influence in drawing air from great depths, might furnish a solution of the facts observed in that city by Pettenkofer and others respecting the relation between ground-water and the prevalence of typhoid.

"The houses in Munich have cesspools beneath them, which are emptied by a hose and steam-engine from time to time. The cesspools are, in many instances, not cemented; and Wolfsteiner mentions that he resides in a large house with many other inhabitants, and calculates that yearly thirty five tons pass into the cesspool under the house; yet in twenty years hardly twenty-five tons have been removed; so that in twenty years six hundred and seventy-five tons of excrement have soaked into the ground. Now in Munich the warm houses help to draw the cesspool air into the rooms, and as the streets are beautifully paved, and no gas can escape through them, the cesspools and houses must be the only ventilating shafts for the soil air."

In this connection may be mentioned a proposition made by a writer in the *Annales d'Hygiene* for watering the streets of cities which are paved with pulverizable material, as also walks in parks and gardens, with a solution of the chloride of calcium in lieu of simple water. This is stated to be economical as well as wholesome, because the first cost is not high, and one watering will last five or six days, while simple water has to be re-applied several times a day in the long, hot days of summer. The application of this process to garden-walks has the incidental advantage of arresting the growth of grass, and thus of saving the cost of the usual method of keeping the walks free from grass. The editor of the *Annales d'Hygiene* states that this method has been tried with success.

Dr. Frankland has recently investigated the conditions under which organic germs pass from sewage into air, and by means of decisive experiments has demonstrated the fact that the breaking up of minute gas bubbles resulting from fermentation or putrefaction is a cause of the suspension of solid organic particles in the air. If, therefore, through the stagnation of sewage, or constructive defects which allow the retention of excrementitious matters for several days in a sewer, putrefaction sets in, then gases are generated, and the dispersion into the air of zymotic germs is very probable. It is, therefore, of the greatest importance that foul liquids should pass rapidly and freely through drainage-pipes and sewers, so as to secure their discharge before putrefaction sets in.

It has been known for some years that green wall-papers deteriorate to a hurtful and even dangerous extent the atmosphere of sleeping rooms by reason of the arsenical salts with which they are colored. It now appears, according to a statement in the *Sanitary Record* of March 15, 1877, that paper-stainers have found it an unusually profitable practice to use arsenic in even the palest white drawing-room papers, especially those which have an enamelled ground.

In the *Sanitary Record* for April 13, 1877, Mr. Baldwin Latham discussed the importance of hydro-geological surveys from a sanitary point of view; and later in the year, namely, on the 5th of October, read a paper on the same subject before the Sanitary Institute of Great Britain, at Leamington. He cited several instances in which a knowledge of the direction of the underground current, as determined by the geological formation,

would have prevented contamination of wells by appropriate location of cesspools. In view of numerous specifications given by him it is evident that the assignment of a minimum "protective distance" of four or five hundred yards from a cemetery gives no guarantee of safety if the well should be below the contaminating source as to the direction of the subterranean flow. This recalls the outbreak of typhoid fever at Lausan, near Basil, in Switzerland, a few years ago, when it was proved by positive experiments that the contaminating material was carried two miles under a hill to a village on the opposite side, this village being on the direct line of flow of the subterranean water, and at a lower grade.[1]

The now well-known influence of dampness of soil in causing consumption, as demonstrated by Buchanan in England, and Bowditch in America, makes it a matter of the first importance to select suitable ground for dwelling-houses. When in towns and cities such a selection is precluded by the necessity of occupying a definite spot, and the retentiveness of a stiff clay soil does not admit of effectual underground drainage, it is held by the best sanitary architects that dwellings should be constructed without cellars, and be well raised above the ground, with a free ventilation between the surface and the ground-floor of the house.

As to other sanitary requirements in connection with dwellings I need say no more than that the greatest trouble relates not so much to the want of a clear conception of what is required as to the practical difficulties of execution. They are questions of

[1] "In conducting a hydro-geological survey it should be thoroughly borne in mind that though, as a rule, water underground follows the natural inclination of the surface of the district, often there are exceptions to this rule. There are, also, circumstances which may modify the flow of water; such, for example, as the abstraction of a large volume of water at a particular point by pumping from a well, which well would become the centre for a drainage-area, extending, in all probability, for a considerable distance from the well; or in some flat districts the elevation of the water-line of a river in time of flood, may reverse the direction of the flow of the underground water, unless, as is the case with some wells which are known to be tidal, the volume of water flowing to the river is very large. It is scarcely possible in a town where there are so many points for pollution, to so locate a surface-well as not to be affected by some of them. The use of the water from surface-wells within a town ought, therefore, to be prohibited for domestic purposes. It is rather singular that, while measures are being adopted for the prevention of the pollution of streams flowing on the surface, and which, by the way, have never been traced to be the cause of disease, no one has thought of the great evils that have resulted and will result from the pollution of the underground sources of the water-supply."—Baldwin Latham, loc. cit.

architectural and sanitary engineering rather than of scientific principles. All concede the great importance of thorough ventilation of sewers and soil-pipes, but sanitary engineers still differ widely as to the safest and most effective means of accomplishing this object.

I am not aware that any real progress has been made towards a definite solution of the great sanitary problem of the purification and final disposal of sewage, since the elaborate reports of Dr. C. F. Folsom, Secretary of the State Board of Health of Massachusetts, in the 7th and 8th Annual Reports of the Board. From these exhaustive reports it appears that purification by the oxidizing influence of a porous soil is the only process which gives satisfactory results, and that these results may be almost complete if the operation be well conducted.

The common practice in America of discharging the sewage of cities and towns into the nearest river or running water will not long be tolerated where other populations are dependent wholly or in part on such streams for their water-supply. Massachusetts has already followed the example of Great Britain in legislating against the pollution of rivers.

HOW TO PREVENT THE SPREAD OF CONTAGIOUS DISEASES.

It is needless to say that there are almost insuperable difficulties in the way of stamping out the infectious diseases which yet are theoretically preventable. In addition to the obstacles arising from false ideas of economy intensified by ignorance and sloth, we have to encounter yet graver difficulties in the fact that nearly all the infectious diseases are subject to epidemic as well as local influences, and though the latter are amenable to sanitation, we know too little of the laws which govern the former to be able to attack them with much probability of success. And yet the very remarkable manner in which they recur at somewhat regular periods may and should serve to aid us in our efforts to prevent their spread. Dr. Ransome, of Manchester, England, has presented some striking illustrations of the cycles of recurrence of the epidemics of zymotic diseases; diseases which by their very class name might be supposed to depend exclusively on local causes. In a lecture delivered by this gentleman before the National Health Society, in May, 1877, he exhibited diagrams representing graphically cycles that have

been observed for upwards of a century by smallpox, scarlet fever, and measles. They displayed in a remarkable manner the tendency of these diseases to recur in certain definite periods, and they showed moreover the difference between the course followed in densely peopled England and in the sparse population of Sweden. The period of recurrence for smallpox appears to have been six or seven years, that for measles five or six years, and scarlet fever has a greater wave-length of fifteen to twenty years, with lesser undulations of about five years. The sudden interference with the cyclical period of smallpox brought about by vaccination is clearly shown, so that Dr. Ransome is inclined to think that to point to these diagrams would be a sufficient answer to any anti-vaccinationist who presumes to doubt the efficacy of this means to arrest the spread of this horrible disease. "We ought," he says, "always to be on our guard against these disorders, but a knowledge of the curve that they usually follow ought to assist us in preparing to deal effectually with them at their periods of greatest prevalence."

He also considers as suggestive and instructive the curious fact that those epidemics which chiefly affect certain organs of the body are most common at the season of the year when other non-specific affections of those organs are prevalent and in places most favorable to those diseases. Thus, whooping-cough and measles generally attain their highest prevalence as epidemics in the winter and spring when bronchitis and other affections of the air passages abound. Scarlet fever nearly always spreads most rapidly in autumn, and occasionally in the spring, and at both these seasons relaxed and ulcerated throats are most common. And so for cholera and enteric fever, of which the maximum rate of prevalence is in summer and autumn respectively, in accordance with an observation of Murchison that "circumscribed epidemics, or the ordinary autumnal increase of fever, are often preceded by a great increase of diarrhœa." "It may be," as suggested by Dr. Ransome, "that the weakness of particular organs affords an opportunity for the entrance of the epidemic poison which has an elective affinity for those organs, and we may be able to meet and to ward off attacks of epidemics by paying especial attention to the prevention of diseases that weaken or predispose those organs to attack." Dr. I. Sinclair Holden, in a paper read at the Congress of the Sanitary Institute of Great Britain, at Leamington last October, aimed to show that careful

investigation could usually discover the rule of law under all the apparent irregularity and caprice of contagion. He referred particularly to scarlatina, which had been mainly studied in connection with severe cases. The mild cases, *always the most infectious*, were not so closely scrutinized and kept under sanitary supervision. Whenever an outbreak occurred a mild case was at the bottom of it, whereas where isolated cases cropped up and led to no other cases, they were sure to be severe cases. The rationale given was that the mild cases shed an almost invisible poison-dust from the skin, and were let loose on society in this stage, and because they were never really ill, little restraint was maintained. Hence the importance of converting the dry scaly dust into a moist controllable form, by frequent rubbing of the whole body, as soon as the faintest rash appeared, with oil or lard, this injunction being most imperative in the class of mild cases.

Two members of this Association, who have won honorable distinction by their zealous devotion to sanitary studies, have recently placed on record their apparently successful experience in attempts to control zymotic disease by prophylactic treatment. Dr. Ezra M. Hunt, my immediate predecessor as Chairman of the Section of State Medicine and Public Hygiene, has published two interesting and suggestive articles in the *Medical Record* of New York for Sept. 22 and Dec. 8, 1877, on the prevention of scarlet fever and diphtheria by individual sanitation: Dr. L. D. Waterman, of Indianapolis, in a short article in the *Practitioner* of London, for March of the current year, states that his use of sulphurous acid administered on antizymotic principles has been strikingly successful.

Dr. E. J. Syson, a medical officer of health, in a well written paper on the "Antiseptic Treatment of Zymotic Diseases," read before the Hunts Medical Society and published in *Public Health* for Nov. 9, 1877, puts in a strong claim for antiseptic medicine, and states that in 1866, he supposed himself to have discovered that it was impossible to vaccinate where the patient was thoroughly under the influence of arsenic, which induced him subsequently to see how far arsenic would exercise antagonistic influence to smallpox. In three cases the results quite confirmed his anticipations. Yet more decisive results were obtained by the use of the same agent, both as a curative and a prophylactic, in cases of rinderpest.

Dr. E. B. Baxter, whose experimental studies of certain dis-

infectants as recorded in the Report of the Medical Officer of the Privy Council and Local Government Board for 1875, fully warranted his announcing some positive and some negative conclusions of more or less value, has quite recently expounded his latest views on the "Theoretical Aspects of Disinfection" in a paper read to the Society of the Medical Officers of Health. He justly, in my opinion, regards the evidence of the causal relation of septic microzymes to putrefactive changes to be now too strong to be disputed, which could not have been affirmed with certainty so late as three years ago. While abstaining from dogmatic assertions as to the completeness of the proof of the parasitic theory of communicable disease and as to the vegetable nature of contagium particles, he yet recognized certain analogies between these contagia and the septic microzymes, and adds that, "if it could be empirically ascertained that both stood in much the same quantitative relation to certain destructive chemical agencies, one might provisionally accept the generalization that germicidal was a measure of disinfectant power." The results of his first indirect experiments were not encouraging; the germs of septic microzymes appeared far more susceptible than the contagia to the operation of destructive agencies. The microzymes had all been held in Cohn's solution, which was neutral in reaction, whereas the virulent liquids are mostly alkaline, charged with albuminous principles and saline ingredients. The more nearly the medium containing the infective particles was made to approach the type of pure water, the more easy became the task of disinfection: on the other hand, by suspending septic microzymes in albuminous and highly alkaline liquids, they became less amenable to germicidal influence. Then germicidal power might be taken as a measure of disinfectant power provided full allowance be made for the protective influence of media.[1] Holding that what might be called the "intrinsic" resistance of contagia to destructive agencies was insignificant in comparison with that conferred upon them by their media, and remembering their ten-

[1] According to recent researches of Dr. Anders referred to by Dr. Sanderson in his lectures on "the Infective Processes of Disease," delivered in the early part of the current year, in the Theatre of the University of London, "it was shown conclusively that doses of chlorine and salicylic acid which were more than sufficient to destroy, and actually did destroy, the vitality of the living organisms present, left the virulence of the liquid unimpaired."—*British Medical Journal*, Jan. 5, 1878.

dency to elude our efforts by becoming scattered, it seemed that the success of the operation in any given case must depend upon the knowledge (1), of the media in which the infective particles were contained; (2) of the laws which governed their dissemination or propagation.

This paper was followed at the same meeting by one "On the Practical Aspects of Disinfection," by Dr. Seaton, in which it was shown that for bedding and clothing dry heat was the only disinfectant to whose action such articles could be conveniently subjected. Dr. Seaton described a self-regulating apparatus of Dr. Ransome, which consisted of a chamber heated by gas to a temperature of about 250° F. Should this be exceeded, a fusible plug was melted and shut off the gas. Some blankets and quilting from the Infectious Diseases Hospital were exhibited, which had been in use for three years, and had been baked over thirty times without apparent injury.

If, then, the contagia were propagated only by *continuous* development, whether in the infected organism or in the excretions, in accordance with Budd's theory in regard to the contagium of typhoid fever, the outlook would, indeed, as suggested by Mr. Baxter, "be very hopeful, and we might even anticipate an approach to the *perfect* fulfilment of the work of disinfection, by subjecting all matters immediately after their removal from the affected person, and before any dilution or admixture, to the full influence of one or other among the destructive agencies at our command."

This result is feasible, and ought to be realized in the case of smallpox, scarlatina, and possibly some other zymotic affections, but, unhappily, this is not the case, as, I believe, with typhoid and some others. That typhoid fever—I will not say may be *generated* spontaneously, for I believe it to depend on a living contagium, and I do not believe in the possibility of a spontaneous generation of living organisms—but that it may appear in a given place otherwise than by direct, *i. e.* recent, importation by either persons or fomites has, I think, been placed beyond question.[1] In some of these cases it would appear that the contagia may be

[1] It would surely be unwarrantable to say that a case of typhoid had originated spontaneously, that is, that the contagium had *originated de novo* in the spot where it was manifested, until every possible channel by which it might have been introduced from without, either at or near the time, or at some not very distant epoch, had been excluded.

wafted in clouds of definite width over long distances, giving rise to true wide-spreading epidemics, while in others there is reason to believe that germs long buried may be disinterred and be awakened into activity by upturning of the soil. It is obvious that in such cases artificial disinfection would be for the most part practically inoperative. For such modes of approach we can only oppose to the morbific agent such defensive measures as cleanliness, good ventilation, drainage, the use of uncontaminated water, wholesome food, and a brave cheerfulness of spirits.

ON THE THEORY OF CONTAGIUM VIVUM.

No other subject within the scope of sanitary inquiry has excited greater interest during the past year than that which is connected with the investigation of the nature of the contagious material of the infectious fevers. The doctrine of a *contagium vivum* has been recently assailed with earnestness and vigor by that able and zealous sanitarian Dr. W. B. Richardson. Nevertheless, that doctrine has been steadily gaining ground, not only in the increasing number of its avowed adherents, but still more in the variety and significance of the analogies by which it is sustained.

It will be remembered that a little more than a year ago Dr. Burdon Sanderson who still holds fast to the doctrine of living contagia, seemed to give up the germ theory, in that he denied to contagium the attribute of structure in the anatomical sense, and maintained that "so far as the morphologist is concerned, germs have quietly gone out of court, and given place to things which are ultra-microscopical, to molecular aggregates, of which all that we can say is, that they occupy the border-land between living and non-living things." I have elsewhere commented upon the weakness of the argument implied by this statement, as really resting upon no other basis than the limits of microscopic demonstration, and I cited the admission of its author that "after all contagium resembles an organism much more than it resembles a chemical body, for its characteristic behavior is such as, if it had structure, would prove it to be living. What, he asks, is more characteristic of living protoplasm than that while maintaining its own integrity it alters the surrounding medium?"

Assuming that the matter of contagium is identical with the "germinal matter" of bacteria, Dr. Sanderson endeavors to fortify

his position as to the absence of morphological characters by adverting to the fact that this germinal matter resists destructive influences which are fatal to the adult bacteria. Prof. Tyndall has shown that this argument is utterly without validity. He adverts, in a paper published in the proceedings of the Royal Society of June 21, 1877, to the well-known fact that the contagium of splenic fever appears under two forms, one of which is fugitive and readily destroyed, the other persistent and destroyed with difficulty. It had been shown by Koch, and also by Cohn, that the fugitive contagium is the fully developed organism of *Bacillus anthracis*, while the persistent contagium is the spore of that organism. So likewise the observations of Dallinger and Drysdale establish a great difference between the death temperatures of adult monads and monad germs. With a magnifying power of 5000 diameters these gentlemen demonstrated the production of spores by adult organisms some of which organisms did not exceed the $\frac{1}{5000}$ of an inch. The young spores were watched through to the adult condition, and it was found that while a temperature of 140° F. was sufficient to cause the death of the adults, the spores were able to grow even after having been heated to 300° F. for ten minutes. "Can it be philosophical," asks Mr. Dallinger, "with the life history of bacteria still unknown, to assume it is a different method of propagation?"

Then as to the ultra-microscopical minuteness of the contagious particles. Prof. Tyndall argues with pertinency and force that between the microscopic limit and the true molecular limit there is room for infinite permutations and combinations, and he invokes the uniformity of nature to sustain his position that "a particle, whether great or small, which when sown produces a plant, is proved thereby to be the germ of that plant," certainly as defensible and rational a position as that of Dr. Sanderson that a particle, however fruitful it may be, ceases to be a germ and "dwindles to a molecular aggregate" when it becomes ultra-microscopical.

As to the bearing of this discussion on the question of the germ theory, after saying that, "in order that any particle may be rightly termed a disease germ, two things must be proved concerning it, namely: first, that it is a living organism; secondly, that, if it finds its way into the body of a healthy human being, or of an animal, it will produce the disease of which it is the germ," Dr. Sanderson, replying to Prof. Tyndall, in the Proceedings of the

Royal Society, No. 184, Nov. 22, 1877, contends that there is only one disease affecting the higher animals in respect of which anything of this kind has been proved, namely, splenic fever of cattle; in other words, there is but one disease in which the existence of a disease germ has been positively established. Now, comparing such a germ with the germinal particles under discussion, he thinks there is but little analogy between them, inasmuch as the latter are not *known* to be organized, and, he adds, "they have no power of producing disease, for it has been proved by experiment that ordinary bacteria may be introduced into the circulating blood of healthy animals in considerable quantities without producing any disturbance of health."

This last statement of the innocuousness of ordinary bacteria may be conceded, but it is certainly somewhat irrelevant in this connection. Cohn has successfully maintained that there are numerous species and varieties of microzymes, and it is surely illogical to infer the absence of morbific qualities in all from the demonstrated innocuousness of one or more of the species. In splenic fever, as we have seen and as Dr. Sanderson admits, we have positive evidence of the causal connection between the presence of Bacillus anthracis in the blood of the infected animal and the phenomena of the fever. The inference that there may be a similar cause in other infectious fevers, where ocular proof has not yet been given, is supported by numerous and very striking analogies, the force of which cannot be weakened by Dr. Sanderson so unnecessarily reminding us that there is no proof, that is, no positive ocular demonstration, of the actual presence and operation of such a cause. Let him point out errors, if there be any, in the alleged analogies. Otherwise we are entitled to rely upon them as constituting valid data for a legitimate provisional theory.

Dr. Sanderson is himself fully aware that, however innocuous the germs of ordinary bacteria may be, there are other particulate bodies which, having derived infectiveness from some particular source of miasm or contagion, impart a very definite morbific action to air or water.[1]

[1] Dr. Burdon Sanderson, alluding, in his recent lectures "On the Infective Processes of Disease," to its having been suggested that he had changed his views of late as to the necessity of the agency of septic organisms in producing infectious virus, declared with emphasis that he still believes that "to the chemical processes which give rise to the production of infective products, these minute

He moreover denies with emphasis that any experiments of Bastian or others have proved the possibility of spontaneous generation, citing his own experiments and those of Samuelson, Pfluger, and Prof. Tyndall in opposition to the inconclusive ones of Dr. Bastian. It was shown by the former gentleman, as it had been shown several years ago by the late Dr. Jeffries Wyman, that an extension of the time during which the boiling temperature was maintained, or a slight elevation of the temperature above the boiling point, would in all cases completely sterilize any organic infusion. More recently Prof. Tyndall has thrown a flood of light on the cause of the diversity of results obtained by different persons, or by the same party at different times, in attempts to sterilize organic infusions. I have already alluded to the trustworthy observations of Dallinger and Drysdale on the remarkable difference between adult monads and their demonstrable spores in respect of the power of resisting heat. It occurred to Prof. Tyndall that, as, doubtless, a similar difference existed between the finished organism and the germinal particles in the case of bacteria, so too the power of extreme resistance exhibited by the germ would probably be greatly lessened the nearer the germ is to its final sensitive condition. Seeds soften before and during germination, and, assuming that this takes place in the germinal particles as preparatory to their development into the mature organisms, Prof. Tyndall determined to apply a crucial test. "An infusion infected with the most powerfully resistant germs, but otherwise protected against the floating matters of the air, is gradually raised to the boiling point. Such germs as had reached the soft and plastic state immediately preceding the development into bacteria would thus be destroyed. The infusion is then put aside in a warm room for ten or twelve hours. If for

organisms are not only accessory, but necessary. I have always maintained that the facts we already possess relating to the development of organisms belonging to the same group as bacteria, in association with pathological processes, are such as to make it impossible to doubt that such organisms exercise an important part in these processes; but, as regards the theory that the common air we breathe is constantly charged with infective organized particles, *apart from and without reference to its having previously come in contact with any specific source of contagion;* and that when a wound goes wrong, it does so because these ordinary air particles find their way into it, I have never neglected any opportunity of saying, that, to the best of my judgment, such a theory is based on what I venture to think the misapplication of an analogy, than on the direct observation of what actually goes on in the wound itself."—*British Medical Journal*, December 22, 1877.

twenty, we might have the liquid charged with well developed bacteria. To anticipate this, at the end of ten or twelve hours we raise the infusion a second time to the boiling temperature, which as before will destroy all the germs then approaching their final development. The infusion is again put aside for ten or twelve hours, and the process of heating is repeated. By this method of discontinuous and repeated applications of heat we kill the germs in their order of resistance and finally kill the last of them, by a temperature no higher than that of boiling water at the ordinary pressure, and maintained for only a few minutes at a time. "No infusion," says Prof. Tyndall, "can withstand this process if it is repeated a sufficient number of times. He applied it to infusions of a great variety of substances, including those most tenacious of life. Not one of them bore the ordeal. These results were clearly foreseen before they were realized, so that the germ theory fulfils the test of every true theory, that test being the power of prevision."

The attack of Dr. Richardson on the germ theory of disease is far more uncompromising, his views being in direct opposition to the idea of there being a living and self-multiplying contagium. He contends that the poisons of the various communicable diseases are modified *secretions*, and thus, though products of living organs, are not themselves living, but really dead matter, and that different diseases are caused by secretions of different glands.

These views relative to the glandular origin of contagious diseases were first promulgated in 1867, and, with the single exception relating to his alleged success in isolating the poison of septic matter which he regards as a type of the contagia of all the communicable diseases, I believe that his twenty or more propositions asserted in his recent address as President of the Sanitary Congress at Leamington, have failed to meet with acceptance either on the part of the general medical public or of special investigators in the same department of pathological inquiry.

Dr. Burdon Sanderson has shown, by his experiments on the propagation of infective inflammations, that, in cases of putrid intoxication caused by the injection of septic matter from which the septic organisms have been previously separated, if the dose be not so large as to kill the animal in a few hours, there is usually a rapid recovery, *the septic poison not having the slightest tendency to multiply in the infected body*, provided that the access of microzymes be excluded. This presents a most remarkable

contrast with the effects of the introduction of the contagia of the zymotic diseases, and the difference is full of significance. In like manner the observations of Dr. Satterthwaite in concert with Prof. Edward Curtis, as reported in the paper read by the first named gentleman before the International Medical Congress in Philadelphia, had reference chiefly to the poisonous matter of putrid infusions, and only showed what Burdon Sanderson had already established, that this matter capable of producing intense sepsis is a chemical and not a living substance. Nevertheless, the latter has established another fact on equally incontestable experimental evidence, namely, that this chemical poison is the product of bacterial activity and cannot be otherwise manufactured. The error of the germ-crazy folks, if error there be, consists, according to him, "not in stating that bacteria are of pathological importance, but in asserting that because A produces B, and B produces C, therefore C cannot be produced unless A is present. It would be erroneous to say that the yeast plant is the agent in the production of the evils of intemperance, and it is a mistake to say that bacteria are the agents in the production of septicæmia, but just as, if there were no yeast plant there would be no drunkenness, so, if there were no bacteria there would be no septicæmia."[1]

[1] "The septic poison we have seen to be an exclusive product of bacterial development, a product which bacteria are capable of manufacturing from unorganized and perfectly harmless material, a product which, although incapable of passing through certain kinds of filters, is soluble in the ordinary sense. Pathologically, we have seen that it does not act the part of a specific contagium; that in order to the production of its morbific effects, a sufficient quantity must be introduced into the circulation; and further, that the intensity of the effect is proportionate to the quantity introduced; so that if the amount be not too great, the tendency is to a favorable, not to a fatal termination. But every one knows that there occur from time to time in clinical experience instances of a sort of septicæmia of a more virulent kind; cases for example of septic peritonitis, in which the quantity of the agent required to produce the fatal result is not measured by drops or grains, but (if one may so express one's self), in homœopathic doses; cases in which we at once recognize that we have to do, not with a poison of which the effect is determined by its quantity, but with a ferment of which the destructiveness to life is chiefly dependent on the rapidity of its development."

The inevitably fatal result of these malignant forms of septicæmia preceded by "diffuse suppurative infiltration of the cellular tissue" (pyæmia) surrounding the gateway of access by which the poison was introduced, and by delirium and collapse, "being partly, perhaps, due to the direct influence of the specific contagium, but principally to the enormous development in the organism of the ordinary septic poison."—Burdon Sanderson, Lectures on the Infective Processes of Disease, *British Medical Journal*, Feb. 9, 1878.

While thus recognizing the fact that in simple septicæmia the microzymes are not the direct agents which produce the pathological results, we may justly enter a protest against the graver error of those who "maintain, in the face of all the experimental investigations relating to the subject during the last few years, that these organisms are without pathological significance." As Dr. Sanderson pertinently and justly remarks, "if these infinitely minute organisms are present in very intensely infective inflammation, we may be quite sure that they stand in important relation to the morbid process." He has further shown that bacteria grown in Pasteur's cultivating liquid, there being no putrid albuminoid matter present, are for the first crop inert, but eventually a product is obtained which possesses all the virulence of putrescent animal or vegetable infusions, a possible explanation being that the liquid becomes charged with the excretions of the bacteria or with the products of the decomposition of dead bacteria.[1]

Now, as to the doctrine of a specific contagium vivum for each of the specific fevers, I think it a sufficient answer to most of the objections urged against this doctrine to remark that, however plausible some of these objections may appear, they are at once refuted by the conceded positive demonstration of such a contagium in a single case, namely, splenic fever. Koch, having ascertained that the *Bacillus anthracis* produces spores when grown in the cultivating liquid, proceeded to test the pathogenic activity of rods and spores as thus produced. The inoculations of either rods or spores into a small incision in the skin of a mouse produced splenic fever in every instance. If the tested material caused no development of rods and spores in the incubator, it failed to produce splenic fever by inoculation. "Proof," says Dr. Roberts, "could go no further; the infection absolutely followed the specific organism: it came with it, it went with it. These observations were repeated with the strictest precautions at the Physiological Institute at Breslau, under the eyes of Prof. Cohn

[1] Concurrently with this acquisition of a toxic quality by the liquid, there is a marked difference in the appearance of the first and the subsequent crops of bacteria, "the former consisting mostly of rods (becteria proper), the latter of spheroids or micrococci. They differ not only in form, but also in the fact that, whereas the rods are endowed with great mobility and appear to act independently, as if each had a consciousness of its own, the latter are held together in masses by interstitial substance (gloea) and usually form a scum or pellicle on the surface of the liquid."—Burdon Sanderson, *British Medical Journal*, Jan. 5, 1878.

and other competent observers, who fully corroborated their exactness."

The proof is likewise nearly complete in the case of relapsing fever. It is now generally known that the discovery by Dr. Obermeier of Berlin of minute spiral organisms in the blood of patients suffering from relapsing fever has been fully confirmed by late observers. An elaborate monograph on this subject by Dr. Heydenreich of St. Petersburg is quoted by Dr. Roberts in his Address in Medicine before the British Medical Association in August, 1877. It is based on 46 cases which were studied with the most minute care, the blood being examined and the temperature being observed from two to six times each day. Altogether over a thousand examinations of the blood were made. Now the spiral organisms are found during the paroxysms, they disappear at the crisis and are absent during the apyrexial periods. The fever is easily communicated to a healthy person by inoculation with the blood of a patient suffering from the disease, but the blood is only infective during the paroxysms when it contains the organisms, but not at the crisis nor during the apyrexial periods when they are absent. None of the fluids or secretions of the body except the blood are infective, all of which seems to show that the virus is associated with the spiralla.

The fact that the organism lives in the blood in this fever furnishes thus a perfectly satisfactory explanation of the absence of local lesions which distinguishes it from the eruptive fevers. It does not so satisfactorily explain another distinctive peculiarity, namely, the lack of a protective influence of a first attack against a recurrence of the disease, although it is urged by Maclagan that it is reasonable to believe that a permanent impression may be more readily produced on a formed and stable organ than on a constantly changing fluid like the blood, and that a contagium which finds its second factor in an individual organ or tissue is, therefore, more likely to produce a permanent impression than one which finds its second factor in the circulating fluid.

These facts, relating to splenic and relapsing fevers as furnishing a positive demonstration of the doctrine of contagium vivum in its application to those particular diseases, sweep away the objections urged by Dr. Richardson and others against that doctrine in general, and leave us free to accept general analogies as legitimate data for making a provisional assumption of a similar con-

tagium in the case of other infectious fevers in which disease-producing organisms have not yet been detected.[1]

The theory which ascribes the phenomena of the infectious fevers to the agency of living parasites had floated before men's minds in a vague way for many centuries. It was not, however, until about the middle of the present century that there had been observed a sufficient body of appropriate facts to entitle it to be ranked as a legitimate hypothesis, when Henle, in Germany, Dr. J. K. Mitchell, in Philadelphia, and a Dr. Cowdell, in England, nearly about the same time, and quite independently of each other, promulgated very pronounced opinions on the subject, and defended them with an imposing array of striking facts and ingenious and plausible arguments. Liebermeister says of Henle, that he elaborated the theory of a contagium vivum in 1853, with as much modesty as thoroughness, though even as early as 1840 he had maintained it with convincing logic.

Dr. Mitchell's lectures on the Cryptogamous Origin of Malarious and Epidemic Fevers, delivered at the Jefferson Medical College during the session of 1846–47, and subsequently published in compliance with the request of the class, attracted general attention and elicited strong expressions of commendation from distinguished members of the profession, especially from the late Dr. Daniel Drake, then engaged in the preparation of his great work "On the Diseases of the Interior Valley of North America." About a year later, and apparently without any knowledge of Dr. Mitchell's essay, Dr. Charles Cowdell, of London, published a "Disquisition on Pestilential Cholera: being an attempt to explain its phenomena, nature, cause, prevention, and treatment, by reference to an extrinsic fungous origin." A contemporary critic in the *British and Foreign Medico-Chirurgical Review*, in a commendatory notice of Dr. Cowdell's book, advised him to extend his hypothesis to all epidemics, suggesting that "he would, perhaps, find yellow fever and the plague still more to his purpose than cholera." It was not, however, until after the publica-

[1] "In an argument of this kind it is of capital importance to get hold of an authentic instance, because it is more than probable—looking to the general analogy between them—that all infective diseases conform in some fashion to one fundamental type. If septic bacteria are the cause of septicæmia, if the spiralla are the cause of relapsing fever, if the *bacillus anthracis* is the cause of splenic fever—the inference is almost irresistible that other analogous organisms are the cause of other infective inflammations and other specific fevers."—Dr. Wm. Roberts, *Medical Times and Gazette*, August 11, 1877.

tion of Pasteur's researches on fermentation and putrefaction, within the past twenty years, that the germ theory, previously regarded as at most a plausible conjecture, began to be treated as a serious pathological doctrine, being warmly advocated by leading minds in the profession, though as earnestly opposed by others of equal standing and authority. The subsequent discovery of specific organisms in the blood and tissues of animals suffering from splenic fever, and in the blood of human beings affected with relapsing fever, gave an immense impulse to the spread of this doctrine, which now counts among its adherents many of the leading minds of the profession, and which is supported by analogies of great variety and significance. The latest systematic exposition of these analogies which I have seen is given by Dr. Roberts, of Manchester, in an admirable address before the British Medical Association, held in that city in August of last year. This address has been published *in extenso* in the *British Medical Journal*, and in the *Medical Times and Gazette*, of London, August 11, 1877, and contains, besides a summary of previous observations by others, some original experiments of the author himself. The object of some of these experiments was to indicate a curious resemblance between the action of yeast and the phenomena of a contagious fever, and of others to confirm the conclusions of Pasteur and Tyndall, that organic matter has no inherent power of generating bacteria, and no inherent power of passing into decomposition, but that bacteria are the actual agents of decomposition, and that they owe their origin exclusively to parent germs derived from the surrounding media. In addition to the experiments relating to this latter point he further fortifies his conclusions by an argument based upon a striking generalization which is, I believe, at least in this application of the fact, original with himself. "Saprophytes," under which general designation he includes all the organisms associated with the decomposition and decay of organic matter, "are," he says, "as is well known, destitute of chlorophyll, and like all such plants, they are unable to assimilate carbonic acid. They obtain their carbon exclusively from more complex compounds which have been prepared for them by pre-existing living beings." This, of course, was a familiarly known fact, but Dr. Roberts has sagaciously drawn the conclusion that it is manifestly impossible that the primordial forms of life could have belonged to this group; "for if we throw ourselves back in

imagination to that remote era when life first appeared on the globe, we should find ourselves in a purely inorganic world—amid conditions in which saprophytes could not possibly live nor obtain nourishment. The special function of the saprophytes in the order of nature is to destroy, not to create organic matter; and they constitute, not the first, but the last link in the biological chain. For if we regard the order of life as it now proceeds on the earth's surface, we may describe it as beginning with the chlorophyll body, and ending with the saprophyte. The chlorophyll body is the only known form of protoplasm which obtains all its nutriment from inorganic sources. . . . . If ever I should be privileged to witness an abiogenic birth, I should certainly not expect to see a saprophyte; I should rather expect to see a speck of protoplasm slowly formed, without definite shape or dimensions, and nourishing itself, like the chlorophyll body, on a purely mineral diet. The more one reflects on this subject, the more clearly does it appear that the spontaneous origin of saprophytes is impossible. Speaking as an evolutionist, I should rather infer that saprophytes were a late development, probably a degradation from some algal forms which had found their profit in feeding on waste organic matter, and which gradually lost their chlorophyll through want of use, and with it their power of feeding on an exclusively mineral diet."

It has been shown, both by those who affirm and those who deny the doctrine of a contagium vivum, that there is no constant and easily recognizable distinction between septic organisms which produce the poison of putrid fever and the common bacteria of putrefaction, and it is affirmed by Cohn that the *bacillus anthracis*, the deadly contagium of splenic fever, is identical in form and development with the *bacillus subtilis*, a harmless saprophyte, the only difference he could detect being that the rods of the former were motionless, while those of the latter exhibit movements. The same authority has shown that the so-called spirillum of relapsing fever is really a spirochaete, a genus distinguished from spirillum by exhibiting in addition to serpentine movements, by which they are enabled to change their place, an undulation passing along their whole length which has never been observed in spirillum. He has further shown that the relapsing fever species (*S. Obermeieri*) is almost, if not quite, identical in form and botanical character with the *Spirochaete plicatilis*

of Ehrenberg, a species found in dirty water, from which it is conceivable that it may be taken into the human system.[1]

Those who find in such facts as these evidence of the falsity of the germ theory commit therefore an obvious fallacy, inasmuch as the apparent identity in anatomical characters may be consistent with a complete diversity in pathogenic properties, whether this difference depends upon a peculiar state of the system giving rise to a pathological infectivity as a necessary factor in the production of the disease in addition to the circulation of the living contagium, or whether it depends upon changes in the nature of the organisms analogous to those which are observed in both the animal and vegetable kingdoms in connection with the development of permanent varieties. This latter hypothesis is suggested by Dr. Roberts, who at least makes it clear that it rests upon the assumption of a *vera causa* which is also appropriate to the effect which it is invoked to explain. "If," he says, "contagia are organisms, they must necessarily possess the fundamental tendencies and attributes of all organized beings. Among the most important of these attributes is the capacity for "Variation" or "Sporting." This capacity is an essential link in the theory of evolution; and Darwin brings forward strong grounds for the belief that variation in plants and animals is not the result of chance or caprice, but is the definite effect of definite (though often quite obscure) causes. I see no more difficulty in believing that the *bacillus anthracis* is a sport of the *bacillus subtilis*, than in believing, as all botanists tell us, that the bitter almond is a sport from the sweet almond—the one a bland, innocuous fruit, and the other containing the elements of a deadly poison.[2]

[1] Abstract of Recent Researches into the History of Bacteria: made by and under the direction of Prof. Cohn, of Breslau. By P. Jeffrey Bell, in *Microscopical Journal*, July, 1870.

[2] "The laws of variation seem to apply in a curiously exact manner to many of the phenomena of contagious diseases. One of these laws is the tendency of a variation, once produced, to become permanent and to be transmitted ever after with perfect exactness from parent to offspring. Another and controlling law is the tendency of a variation, after persisting a certain time, to revert once more (under altered conditions) to the original type. The sporting of the nectarine from the peach is known to many horticulturists. A peach-tree, after producing thousands and thousands of peach-buds, will, as a rare event, and at rare intervals, produce a bud and branch which ever after bear only nectarines; and conversely, a nectarine at long intervals, and as a rare event, will produce a branch which bears only peaches ever after. Does not this remind us of the occasional apparent sporting of diphtheria from scarlet fever? My friend, Dr. Ransome,

"Cholera suddenly breaks out in some remote district in India and spreads from that centre over half the globe. In three or four seasons the epidemic dies away and ceases altogether from among men. A few years later it reappears and spreads again, and disappears as before. Does not this look as if the cholera virus were an occasional sport from some Indian saprophyte, which by variation has acquired a parasitic habit, and having run through countless generations, either dies out or reverts again to its original type? Similarly, typhoid fever might be explained as due to a variation from some common saprophyte of our stagnant pools or sewers, which, under certain conditions of its own surroundings, or certain conditions within the human body, acquires a parasitic habit. Having acquired this habit, it becomes a contagious virus, which is transmitted with its new habit through a certain number of generations; but finally, these conditions ceasing, it reverts again to its original non-parasitic type." How admirably this ingenious theory reconciles the observations of Murchison and others on the occasional pythogenic origin of typhoid fever with the doctrine of a contagium vivum and harmonizes them with the deductions of Budd as to the continuous development of this fever, it is needless to insist.

When we thus find innumerable analogies between the phe-

who has paid so much attention to the laws governing the spread of epidemics, relates the following instance: A general outbreak of scarlet fever occurred at a large public school. One of the masters, who took the infection, exhibited diphtheritic patches on the throat. This patient was sent to his own home in Bowden. Six days after his arrival his mother was attacked, not with scarlet fever, but with diphtheria; though there were no cases of diphtheria at the time, neither at the school nor at Bowden."

"Complex cases of mingled scarlet fever and diphtheria are sometimes seen. Similarly the peach tree will occasionally, among a multitude of ordinary fruit, produce a fruit of which one half has the peach character, and the other half the nectarine character."

"In regard to some contagia, such as smallpox and scarlet fever, it might be said that the variation was a very rare one, but also a very prominent one, with little or no tendency to reversion; while others, like erysipelas and typhoid fever, were frequent sports, with a more decided tendency to a reversion to the original type. In regard to some pathogenic organisms, it might be assumed that the parent type had disappeared, and the parasitic variety only remained, just as the wild parents of many of our cultivated flowers and vegetables have disappeared, leaving behind them only their altered descendants."—Dr. W. Roberts, loc. cit.

How aptly does this view explain what used to be called the "epidemic constitution," and the hybrid forms and sub-varieties of eruptive and other fevers, such as some of the varieties of typho-malarial fever, and the occasional blending or else alternations of allied types.

nomena of the contagious fevers, and those connected with the development and life of certain low organisms, analogies so numerous and so close that every peculiarity in the manifestation of the fevers as to the mode of development and spreading will be found to be susceptible of interpretation in terms of the doctrine of a *contagium vivum*, and many of them not susceptible of any other explanation, and that, moreover, a positive demonstration has, it is universally conceded, been given in the case of splenic fever, not to insist upon the almost equally conclusive proof in the case of relapsing fever and septicæmia, nor upon the apparently conclusive demonstration, given by Chauveau and subsequently confirmed by Sanderson and by Braidwood and Vacher, that the contagium of vaccinia and variola consists of transparent vesicles, first recognized by Lionel Beale, not exceeding according to Sanderson the $\frac{1}{20,000}$ inch in diameter, it does appear to me that a very strong case has been made out in proof of the general doctrine in question.[1]

Those who are prone to reiterate the assertion that no positive demonstration has been given of a living contagium in the case of typhoid, typhus, or the malarial fevers, and who seem to take for granted that until such demonstration has been given, it is more logical to doubt, if not to deny, the possibility of such a mode of causation, than to hold it as a provisional hypothesis, forget that ocular demonstration may be absolutely precluded by reason of an ultra-microscopical minuteness of the particles, that, moreover, between the microscopic and molecular limits there s space for countless gradations of beings, and that after all inferential proof may be quite as conclusive as sensible demonstration, in some cases, indeed, much more so, the liability to commit logical fallacies in the one case being balanced, or more than balanced by possible errors of interpretation in the other. What would be thought of the scientist who would doubt, not to say deny, the truth of the undulatory theory of light on the ground that the supposed elastic medium, whose motions are believed to constitute the light of the Universe, is itself invisible, impalpable and absolutely imponderable, that we cannot *demonstrate* its presence nor know anything of its essential nature? The assumption of its existence, suggested by observed analogies between many of the phenomena of light and the known effects of the undulations

[1] British and Foreign Medico-Chirurgical Review, Oct. 1877, p. 393.

of ponderable fluids, not only furnishes a satisfactory explanation of all the previously known facts, but has enabled competent philosophers to predict and thus to discover other more recondite phenomena which had eluded direct observation. No proof could be stronger than this. In like manner the extension of the doctrine of contagium vivum from infectious fevers in which positive ocular demonstration of its presence has been given to others of the same class, in regard to which ocular demonstration may be precluded presumably by reason of the extreme minuteness of the particles, is fully justified by the uniformity of Nature. The doctrine in question thus fulfils every test of a legitimate scientific theory. It assigns a cause which as we have seen is true and appropriate to the effects to be explained, while the facility of its application to the solution of all the phenomena of the infective fevers shows it to be also adequate in extent.